Managing Exploratory Research Projects

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Outline

- Goals for this presentation
- Background information
- Similarities and Differences
- Managing Exploratory or Basic Research Projects
- Concluding Remarks

Goals for This Presentation

- Heighten awareness of the differences or uniqueness of managing an exploratory research project
- Heighten awareness of the importance of exploratory research to NASA and the US
- Offer some items for discussion – *Would appreciate your thoughts*
- Share some insights and lessons learned

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Same Project Management Topics – Different Execution

- Still must MANAGE the project
    - Control and allocate resources and time
    - Hold workforce accountable
    - Produce results
    - Risk Analysis

  - Develop and execute a source selection plan
  - Satisfy the customer
  - Effectively lead the other managers and workforce
  - Technology Transition Plan
- These topic areas are executed differently in Managing Exploratory Research

Some Quotes

- *"Predicting the outcome of worthwhile basic research should not be easy. Serendipitous results are often the most interesting and ultimately may have the most value. Taking risks and working towards difficult-to-attain goals are important aspects of good research management."*
  - OMB on Basic Research

- *"The output of research is knowledge and it is difficult to predict in advance, with any accuracy, the quality, quantity or usefulness of the knowledge that will be generated from any given research project."*
  - NRC of Canada, 1996

TRL 1-3 Definition

- **TRL 1: Basic principles observed and reported**
  - This is the lowest "level" of technology maturation. At this level, scientific research begins to be translated into applied research and development. Examples might include studies of basic properties of materials (e.g., tensile strength as a function of temperature for a new fiber).

- **TRL 2: Technology concept and/or application formulated**
  - Once basic physical principles are observed, then at the next level of maturation, practical applications of those characteristics can be 'invented' or identified. For example, following the observation of high critical temperature superconductivity, potential applications of the new material for thin film devices (e.g. SIS mixers) and in instrument systems (e.g., telescope sensors) can be defined. At this level, the application is still speculative there is not experimental proof or detailed analysis to support the conjecture.
- **TRL 3: Analytical and experimental critical function and/or characteristic proof-of-concept**
  - At this step in the maturation process, active research and development (R&D) is initiated. This must include both analytical studies to set the technology into an appropriate context and laboratory-based studies to physically validate that the analytical predictions are correct. These studies and experiments should constitute "proof-of-concept" validation of the application/concepts formulated at TRL 2. For example, a concept for High Energy Density Matter (HEDM) propulsion might depend on slush or super-cooled hydrogen as a propellant: TRL 3 might be attained when the concept-enabling phase/temperature/pressure for the fluid was achieved in a laboratory.

Project Success or Failure?

- **Too Long? :** The Light Bulb:
  - **It took Edison 2000 steps to invent the light bulb.**
- **Insufficient?:** Heavier-Than-Air Flights:
  - **The first flight lasted a mere 12 seconds**
- **Not Planned?:** Post-it Notes:
  - **Glue failed to meet original requirements**

- **Not Needed?:** Laser Technology:
  - **Took years before even a few practical applications were identified**
- **Waste of funds?:** The Telephone:
  - **Nearly all expert of the time said it was impossible and therefore not worth trying**

An Example from the NASA Breakthrough Vehicle Technologies Project

- What: Disruptive Technologies and Tools to Enable Program Goals
- Why: Creativity and Innovation Crucial to Future Vehicle Capabilities
- How: Discovery, Experimentation, Expert Opinion, Failure, and Extensive Test and Evaluation

Some Examples from NASA Morphing Project

- Project Approach:
  - **Create new technologies –** Actuators, structural concepts, etc.
    - In Response to System Studies and Vehicle System needs
  - **Address application issues –** Electronics, design tools, system integration, etc.
    - What's needed to make the technology work
  - **Assess functionality –** Integrate and test concepts, demo new capabilities
    - Understand the nuances of How, When and Why the Technology works
- **Deliverables: (Often transferred through collaborations, papers, and patents/licenses)**
  - **Technologies that Enable New or Significantly Enhanced Vehicle Capabilities**
  - **Tools to use the technology (hardware, software and guidelines)**
  - **Demonstrated results (the good and the bad)**

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What's the same as other projects in terms of managing exploratory research?

- Fixed Time
- Fixed Resources
- Good Toll Gates/Control Gates Needed
- Boundaries on "Good", "Acceptable" and "Non-Acceptable" must be set
- Importance of
  - Good teamwork
  - Good leadership
  - Good communication
  - Partnerships

- Reviews

What's Different about Managing Exploratory Research Projects?

Uncertainty

- Uncertainty in outcomes of research - "good"/accurate planning is difficult
- Time lag in real benefits of work and funding
- How do you define/measure success?
  - Quantifiably defining requirements
  - "Success" is often not quantifiable by traditional standards
- "Success" may be very different than what was planned (due to discoveries)
  - "Frequent" reprogramming of resources necessary to enable pursuit of recent innovations
- High risk is expected/necessary
  - The least risky approach will often not yield the best results
- Project Management Measurement Differences:
  - Critical Path
  - Cost-Benefit-Analysis
  - Earned Value Management
  - Workforce Utilization
  - Scheduling and Progress Reporting

More on Risk…

- Risk, Part 1: It is <u>high risk</u> to leave our established research area
- Risk, Part 2: We choose several promising paths to <u>reduce risk</u>

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Some Challenges

- How do you schedule Breakthroughs?
  - You don't - You develop a strategy for including them
- Working with Unconventional Technologies and Figuring Out How to Use them Effectively
  - "Beating away" at the conventional approach often won't yield the best results
    - Example: Conventional oven to Microwave
    - Example: Carburetor to Fuel Injector
    - Suggestion: Try not to use the same workforce that is doing product development on the conventional approach
  - Retrofits often dramatically under-use the technologies developed – Plan for unusual demonstrations
  - Balancing the research portfolio

- Plan for multiple approaches to the problem
- Balance the overall risk – Be sure to include a couple "winners"

- How do you justify funding the "Galileos" of the 21st Century?
  - Typically a quantitative justification is desired by funding organizations – Develop the project around a national need or a compelling vision

Some Thoughts/Lessons Learned

- Providing Guidance and a Productive Environment
  - Don't forget the journey (we learn form our failures). Be careful not to overemphasize the target (avoid only defining success around the target).
- Write milestones to allow for discovery (may or may not be acceptable to upper mgt.)
  - **Okay:** Develop n self-healing materials for xyz conditions
  - **Better:** Assess the self-healing capabilities on n to m materials for xyz conditions
  - **Okay:** Demonstrate virtual shaping using synthetic jets for virtual shaping
  - **Better:** Evaluate the feasibility of using synthetic jets for virtual shaping
- Identify Risk, Avoid Mitigating it Too Much
  - Risk and learning are part of the fun and success (test early, test often…)
  - Must continually address the culture of being risk adverse

Stetting Project Level Goals/Objectives within the Project Timeline vs. Developing a Long-Term Vision or Objectives beyond the Project Timeframe:

Need to have a unifying vision (dream?)

- Provide focus and inspiration for researchers
- Communicate the vision frequently (because it may or may not be institutively obvious from the intermediate project outputs)
- Helps in getting funding from upper management
- However, convincing funders that very far out work (funded at "incubator" or seedling level) is useful may be tough

- Unscheduled (and potentially frequent?) cost and progress reviews and adjustments
  - Expect, Have a strategy for and encourage the "A HA's!"
  - It the project is successful there should be some "A HA's every year
  - Be willing to reallocate resources toward new discoveries

- Determining workforce balance
  - "Chemistry" of researchers
  - Not everyone is cut out for exploratory research (out-of-the-box vs. in-the-box thinkers, etc.)
  - Often requires stepping into the unknown
  - Expecting someone to do product development as well as basic research is usually unreasonable (conflicting requirements)

Some Thoughts/Lessons Learned- Accountability

- Still need to define some metrics for acceptable and not acceptable effort = "Success" and "Failure"
- Some thoughts on "success"

- New knowledge gained (by us, students, country)
- New capability (analytical tools or material, etc,)
- Assessment conducted/Functionality determined (Whether the technology worked or not)
- Application issue addressed

- Some thoughts on "failures"
  - Not using the best science available (in sufficient intelligence gathering, inappropriate method, etc.)
  - Not making a good effort towards the research
    - Major plan departures without PM approval
  - Failing to follow established safety or security procedures

Some Thoughts/Lessons Learned- Reviews

- Pjt Mgt reviews no more frequently than quarterly (often twice a year is sufficient)
- Research progress reviews (usually subjective and conducted semi-annually)
  - How close are we? Should we continue on this path?
    - "Third and long" – go for it
    - "Fourth and long" – punt
    - "Fourth and short" - not sure – usually go for it
  - Holds researchers accountable
  - Required skills of the Project Manager
    - Extensive knowledge of the SOA is essential for determining (guessing?) appropriate next steps
  - A good, gregarious and knowledgeable planning team is crucial (technical advisors to the Project Managers)
- Peer Working Groups to informally discuss progress and issues help to avoid getting stuck in a "local minima"

Concluding Remarks

- Managing Basic Research can be extremely rewarding
  - Working at the leading edge of new discoveries is very exciting
  - Project Philosophies: Work with the Best, Stay ahead of the Rest, Challenge Traditions
  - While applying crucial Project Management approaches

- **HELP!**
  - Basic research is **crucial** to the US
  - Some traditional project management policies can be very stifling

- Suggestions and Feedback are welcome!